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THE PRESENT STATE OF THE THEORY OF DISTRIBUTION—DISCUSSION

JOHN BATES CLARK: It is a pleasure to know that Dr. Hollander thinks it worth while to make so vigorous a protest against an excess of theoretical study. With some of us the question that appears to be most pressing is whether, in view of the overwhelming drift toward practical investigation, such study can survive. It is also a pleasure to know that Dr. Hollander wishes for new facts in order that, in the end, we may have theories.

I must demur at two points. I find, first, no such startling contrast between the method of the physicist and that of the economist. In both cases there are the observation of common facts, the tentative formulating of principles, and the ultimate verification of the principles by an extensive collection of facts. I am, secondly, not ready to accept Dr. Hollander's estimate of the value of what theoretical study has recently accomplished. It seems not little but so great as to be a principal cause of the active discussion of methods. When a miner has nearly "worked out his claim," method becomes of increased importance, since with a meagre supply of ore poor processes would bring operations to a stop. A more fruitful policy on the miner's part is to take up a new claim. We have worked out in a large degree, the static field. We have solved such problems as those of the natural standard of value, that of wages, and that of interest. The rich dynamic field is before us, and we must formulate principles governing changes in these standards, and deviations of actual rates from

them. Vast and critical problems of progress, of obstruction, and of conditions of future well-being are pressing for solution and in solving them we shall proceed as we have done in the static division of our field, though we shall find ourselves working veins of far greater richness. We shall observe familiar facts—and there is enough of them for our immediate purpose. We shall tentatively formulate principles and shall later verify them as our knowledge increases. We shall not neglect the principles and blindly pile up statistical figures in an unmanageable mass. Such a collection of facts might come to have a value which would grow smaller as the mass grew larger, and end by becoming so vast and unassorted as to have little value to the practical man, and almost none to the scientist.

ROSWELL C. MCCREA : To begin with, I am going to make the assumption that Dr. Hollander, in the major portion of his criticism, had in mind the marginal productivity theory of distribution. I am very much at one with him in the general implication of his paper that if the validity of the view of the marginal productivity of theorists is really to be tested, it must be done, not by quibbling over details of analysis or over minor shiftings of view-point, but by an examination of the propriety of their logical method, or the truthfulness and completeness of their premises, and of the practical significance of their conclusions. So far as I cannot agree with him, it is largely from the fact that whereas his criticisms are given with an air of certitude, my own are purely tentative, and that I am inclined to put the emphasis on points to which he has given slighter weight. And I must confess at the start that, leaving out of account variations of treatment of

the marginal productivity theory, which it seems to me are due mainly to differences in the circuitousness of the routes by which different writers have ultimately reached the same goal, I cannot find any vital flaw in the main course of the reasoning of the marginal productivity theorists.

As regards method, I am not able to make the sharp distinction between the "metaphysical" and the "analytical" that Dr. Hollander makes. Logically considered, the so-called "analytical" method, it seems to me, is in no wise different *as a method* from the "metaphysical" method so-called, unless it be that with the former more frequent recourse is to be made to verification of steps in the argument by reference to facts that are usually to be found with the latter. The suggestive feature of the term "metaphysical" lies not in what it connotes with regard to method *per se*, but in what it implies with reference to the character of premises. Whatever name we give to the method of productivity theorists, it is like what I cannot help regarding as the significant branch of Dr. Hollander's analytical method, deductive, and as elaborated, particularly in Dr. Clark's case, is apparently the result of an analogy between economics and mechanics. As in mechanics the first task is that of separating those forces which are fundamental from those that are frictional, so in economics influences that are permanent and universal must be distinguished from those that are merely disturbing. At times the parallel seems to be the perhaps truer one of a composition of forces; but since in such case the forces considered are equally fundamental, this figure hardly fits the requirements, and so is probably not the intended one, and I cannot see that there is anything necessarily objectionable in borrowing the method of

another science. In the present instance the possible danger lies, not in the method itself, but in the extreme nature of basal abstractions, and in the too infrequent testing of preliminary conclusions by reference to actual phenomena (the latter perhaps involving a defect of method that may be called "metaphysical"). Dr. Clark makes his abstractions once for all at the start, with the result of forming a picture of economic life in a state of dynamic equilibrium, his so-called "static" state; whereas others, less heroically inclined, make their abstractions as they need them, the net effect being much the same in either case. But in a social science like economics, where causes are so much more numerous and complex than in physics, the danger is that of turning the necessary liberty of making abstractions into the license to make mere assumptions, with the result of establishing as points of departure in theory, not truths stripped of the irrelevant, but fundamentals that are essentially untrue to real life. To say the least, much of recent theorizing has run perilously near the danger line. To assume, rigidly to adhere to the assumption, that "the primitive law which puts a man face to face with nature and makes him dependent upon what he personally can make her yield is still . . . the law of the most complex economy,"¹ and to assume among other things absolute freedom of competition and the entire fluidity of capital and labor, is, strictly speaking, something more than mere isolating of phenomena. It is pure hypothesis. Actual production is not carried on by the use of an abstract homogeneous fund, nor are shares of income drawn from an abstract homogeneous flow; and the typical wage receiver

¹ Clark : *The distribution of wealth*, p. 37.

is not actually paid out of the present product of his labor. The whole point of view is admittedly imaginary and hypothetical.

But granting the truth of this charge, the resulting body of theory is not thereby necessarily discredited. Hypotheses, subsequently proved untenable, have been of great value in the forming of other sciences; why not in economics? Here, as elsewhere, the real tests must come in the attempt to bring resulting conclusions into real touch with the actual facts of business life, and in the closely related attempt to throw light on questions of public policy. For in the last analysis, a body of theory must be judged not only by its truthfulness to existing phenomena, but by the applicability of its conclusions and the character of its consequences in the field of practice. The first of these tasks has thus far been attempted only on a small scale, but in such a way as at least to reveal outlines. And perhaps the most available as well as the most serviceable of these essays is to be found in Dr. Seager's "Introduction to economics." Here the main conclusions involved in the marginal productivity theory are accepted with but brief attention to preliminary analysis; but what is actual in the productive and distributive process is outlined at some length with great skill and acuteness of observation. And yet, in the work of articulating the hypothetical with the actual, the main result has been not so much to prove the fundamental character of static conclusions as to emphasise the divergence between those hypothetical standards and the actualities of economic life. And the same is more or less true of the other expositions of the productivity theory.

The application of the theory to questions of public policy, aside from contradicting the pretensions of

socialistic theory, has proceeded mainly along the line of idealizing static standards. For instance, from the static truth that the wages of a laborer are what can be specifically imputed to him as his product is projected the ethical standard that the wages of labor *ought* to be the value equivalent of the product of labor. But the realization of this, as of related ideals, is dependent upon the existence of competition, and freedom of competition is seen to be no longer possible under a policy of *laissez faire*. Logically, therefore, it becomes the duty of the state to guarantee fair and substantially free competition by the exercise of its regulative function: a rather commonplace end to so long and tortuous a journey, and a conclusion the expediency of which must of course be judged on much broader grounds than those suggested in a theoretical study on distribution.

On the whole, my own position is one of doubt. The acuteness of the reasoning of the exponents of the marginal productivity theory, and the definiteness of their conclusions cannot but make a strong appeal to one who wishes to find some way out of the maze of theoretical discussion. But at the same time, in view of the purely hypothetical character of the fundamental notions and of the extreme refinement of analysis, where corresponding refinements of process are foreign to business life; and out of respect for the notion that economics if anything is a practical science, one cannot avoid the impression either that the theory is somewhere at fault or that one is devoid of scientific imagination. Discarding the latter alternative, it would almost seem as if relevancy to the supposed needs of theory had outweighed considerations of the necessity of relevancy of theory to economic life and practice.

FRANK A. FETTER: The opening paper is commendable in its positive aspect, its emphasis of the importance of collecting and classifying more fully and exactly, the facts of the business world; but is unfortunate in its implication that no worthy work of this kind is being done, and in its belittling of other essential methods of economic study. It dismisses much too abruptly the important services of the historical method. Although as the unique method of investigation in economics that method has yielded somewhat meagre results in the form of generalizations, its service as an auxiliary has been great. It has changed the spirit and broadened the outlook of economic philosophy. It has enabled economics to share somewhat in the rich fruits of the evolutionary doctrine. It has given us the genetic conception of economic problems, making impossible the naïve outlook of a half century ago.

Dr. Hollander's rejection of the theory of distribution "as now current," (by which he doubtless means the theory of Mill as amended by Marshall) comes as a pleasant surprise to those who have for years thought as he now does. Two years ago he believed that there was no "real occasion for radical departure from the traditional treatment" of the theory of distribution, and that "our scientific fathers builded wiser than modern critics are inclined to admit" (*Publications A.E.A.* 3d Series V, p. 207, 209). His recent change of view is however confined to negation, and he has not as yet had either the time or the inclination to inquire what positive contributions have been made to the subject of recent years. In the first bitterness of disillusionment he counsils us to throw away all theories of distribution and, lacking principles, to await hopefully the day when a method, which he admits has

achieved nothing by itself as yet, has supplied our urgent need. Some theory of distribution we must have; it is like the atmosphere which surrounds and pervades all economic inquiry. It is vain for Dr. Hollander thus to decree an intellectual vacuum in the realm of economic theory; he must permit us to have something to fill it.

Some of those who of late have been guilty of "text-book didacticism" had been compelled, like Professor Hollander, to reject the old theory of distribution, but they did not feel driven either to agnosticism or to pessimism. They hesitated to adopt the plan he contemplates,—the abandonment for the present of all systematic teaching, or even thinking, on general economics. Rather they felt that it is for the teacher in each epoch to make an honest attempt to formulate and summarize for his pupils the ascertained truths of his subject and to express these truths so far as possible in terms theoretically consistent and practically applicable. The insistent demand of the public and of the body of students for a periodical restatement of economic thought is proper and healthy, and it is the duty of economists to meet it. For the past twenty years economic text-book writing in America has been lagging far behind the general progress of economic studies.

Dr. Hollander's chief condemnation is visited upon the so-called "metaphysical method," which until of late he loved not wisely but too well. As applied to recent studies, the term metaphysical, or even deductive, is, however, a misnomer. Here the theory of distribution is for the first time conceived of as the problem of value "writ large," and economics as essentially man's thought about things as they affect his welfare and become the objects of his estimation. The aim is

to understand man's thought in the process of evaluating goods, and all means leading to this end are used. Dialectical criticism plays only a preliminary and destructive part; it makes clear when, where, and why fallacious ideas were introduced into economic thought. Is it not scientific ingratitude to reveal one's indebtedness to this reasoning, and in the next breath to stigmatize it as hypercritical? In recent study, moreover, anthropology and history are used to see how men have thought of wealth in primitive conditions and how their conceptions have grown with industrial changes. To a slight degree, introspection,—much more, observation, travel, experience, personal contact with men, statistics, description, knowledge of the market,—aid in interpreting and guide in constructing realistic concepts and a realistic theory of value. The method might perhaps be called primarily psychological, though that describes rather the thing studied than the method. The method is more properly called *synthetic*, and seeks to include with other resources all that is workable in what Dr. Hollander now proposes.

Progress in society or in science is not often furthered by the sudden rejection of all the attainments of the past. Those who will may trace historical continuity in the growth of economic thought, and may see as the writer of the paper does not, that the theory of distribution is today clearer, more consistent, and more serviceable than ever before.

T. N. CARVER: I should agree with Dr. Hollander as to the desirability of a more extended use of the analytical method of political economy, but I should disagree with him entirely in the assumption that any other method has ever been followed by any of the leading economic theorists. What he calls the *a priori* or deductive method is

really the analytical method which he advocates. I think, therefore, that if he had applied the analytical method to the study of the subject in hand, he would not have said what he did about the abstruse and metaphysical quality of the economic theorists of the past. And, moreover, the men whom he has commended as having broken away from the metaphysical and adopted the analytical method are men who have done the opposite. If Walker, for example, had used a very moderate amount of analysis in his discussion of the wage fund theory, he would not have said what he did. That is to say, if he had made a distinction between the source from which wages come and the factors which determine the rate of wages,—this is a simple piece of analysis,—he would never have said that wages are paid out of the future product, though he would probably have said that wages are determined by the present or the future product. The difficulty has not been that economic theorists have failed to use the analytical method, but that they have not applied it rigidly enough. This is a comfortable doctrine for all young economists, for it simply means that there is something left for them to do.

The difference between economics and the physical sciences is not that economists have employed a different method of reasoning, but that they are dealing with materials which do not admit of the same exactness of manipulation. Last night when starting for this meeting I fell in with a philologist on his way to a meeting of the Philological Association, who had with him a machine for the demonstration of a theory of Greek scansion. By means of this mechanical device he hoped to be able to absolutely demonstrate the position which he had been defending. It does not

seem possible to invent any mechanical device which will demonstrate any economic theory, however sound it may be. Such a theory must depend for its acceptance upon a reasonable attitude of mind on the part of economists and the intelligent public. As Lincoln once said in one of his debates with Douglass, it is impossible to work up an argument into the consistency of a corn-cob, which can be shoved down an opponent's throat. In the physical sciences, with their laboratory methods and their instruments of precision, certain things can be demonstrated or thrust down the throats of unwilling believers. Perhaps this is the reason why in the physical sciences a man who comes forward with a new theory, which he claims to be not only a new discovery but to be revolutionary and destructive of all existing scientific theories, fails to get a hearing; whereas in economics we frequently hear not only of the discoverers of new theories or new principles, but we hear the discoverer proclaiming that all preëxisting theories are dead, that we must sweep them all away and start over again, building on the foundation of the newly discovered theory. Such a man could not make much of an impression in the field of physical science, where, by means of mechanical appliances and instruments of precision, certain facts are made demonstrable. In medicine, for example, a man who comes forward with a new discovery, which he proclaims as the destruction of all existing medical theories, is known as a quack.

What we need, therefore, is not a revolution in economic methods, but rather a following out of existing methods—for the existing method in economic theory is the analytical method—and a more general willingness to recognize that there may be certain fundamental

principles in the classical political economy which may very easily be improved upon by new discoveries or new analyses, and at the same time that these new discoveries or new analyses may be quite consistent with the fundamentals of the classical political economy. We shall thus avoid two extremes, both of which are about equally destructive of all science: first, the idea that political economy was completed by Adam Smith, Malthus, and Ricardo, and that all new discoveries or analysis are therefore wrong; and second, the opposite idea that because certain new things are discovered we must therefore sweep away everything that has been done by the economists of the past and start over again, building a new science of political economy.

HENRY R. SEAGER: As one of the recent offenders in the field of text-book writing, I wish to express dissent from Mr. Hollander's view that it is time to call a halt upon this branch of activity. Far from agreeing with him on this point, I am inclined to urge the opposite opinion, that the time is very opportune for every economist, who feels in the least drawn towards text-book writing, to try his hand at the experiment. Writing a text-book is not a life work. At most it means the partial absorption of one's time for a period of three or four years. I believe there is no better way in which an economist can sound the depth of his own knowledge—and ignorance—and lay the basis for later original work than through such an exercise.

Even more important, it seems to me, is another aspect of the question. There is after all no surer way in which most of us can contribute to the advancement of economics than through our students. Every improvement in our apparatus for imparting to them what we

already know is certain to render signal service in advancing economic analysis. Thus, unless Mr. Hollander is prepared to maintain that our present supply of text-books is satisfactory—and the whole tone of his paper contradicts this view—it seems to me that this part of his argument is inconclusive.

Time will not permit more than the briefest reference to other portions of his interesting, if somewhat pessimistic, review of the present situation. While all must agree that present day economists, like their predecessors, are far too little in touch with the actual facts of the industrial world, I must dissent very strongly from the opinion that we are worse off today in this regard than our predecessors. On the contrary, I believe that there has been no time in the development of economics when we have been so conversant with the facts which we are trying to explain.

VICTOR ROSEWATER: In the general scramble to pick flaws in some of the incidental features of Dr. Hollander's very suggestive paper, this discussion, it seems to me, has overlooked one of the main points made by him and one to which every teacher of political economy would be expected to assent. Dr. Hollander's paper is really a plea for greater attention to research work on the part of political economists and an appeal for more funds to be placed at the disposal of the working economists for the prosecution of such studies. Dr. Hollander would be the last to say that the teaching of political economy should be dropped, or that the writing of text-books should be entirely abandoned until after a new body of doctrine is developed upon which all agree; but he has very properly emphasized the need of getting out of the class room occasionally and getting in touch with the actual practical op-

eration of economic forces in the every-day world. A great deal is already being accomplished in this direction, but much more is to be done. While the perfection of economic theory is not to be achieved by any single method, greater familiarity with actual conditions of industrial, commercial, and social activity will surely help on and hasten the search for economic truth and the formulation of economic laws.

DAVID KINLEY: It is a matter of much surprise that one who is so sound a theorist as our friend, Dr. Hollander, could have made so severe an arraignment of the usefulness of theoretical writing in economics. Certainly, his own sound practice as an administrator justifies us in believing that his own theories are sound. We can only suppose, as has been remarked before, that he has become enervated by the climate of San Domingo.

There are two points which have already been brought out—one by Dr. Patton, and one by Dr. Clark—which I wish to emphasize. The first is, that Dr. Hollander's paper is not, as we expected it to be, a criticism of the defects of existing theory, but an attack on theoretical reasoning as such. He seems to throw doubt upon not only the validity but the value of theoretical economics. Now, I want to emphasize the fact that we cannot have sound practice without sound theory. It is all very well for so-called practical men to say, "Go out into the world and get together the facts, instead of staying in your class room and studying and theorizing about them." The truth is that such a near view and such a method of gathering data, together with the complexity of the observations, are more likely to lead us astray than even the method of *a priori* reasoning on assumed hypotheses. True, we

must have our facts, but we cannot always get them in the street or in the shop ; surely not in particular streets or in particular shops. A theory is only the generalized statement of correct practice. If our observations of the facts are correct and our logic is good, the theories must be correct. Now, the difficulty has been that our data are imperfect, and too multiplex for easy and complete analysis. We need more facts as a basis of theorizing, but they must be carefully selected facts ; facts which show something and are not a mere accumulation of data. We need more theory and not less economic practice, and administration will grow better the more perfect our theories.

The second point, which, as I say, was brought out by Dr. Clark, is that our theorizing thus far has been in the way of clearing the ground. We have been dealing with static conditions. The result is that we have not, perhaps, yet come to conclusions which are applicable with any high degree of success to existing problems ; but it is no just ground for criticism that we have not yet discovered a law of wages, for example, which will explain wages in a particular industry or shop. That result, it is to be hoped, will come when we have corrected the theories derived from observations of static conditions so that they will accord with the circumstances of dynamic society.

As a matter of fact, however, it is hardly true that we have not made progress, and very considerable progress, in the theorizing of the past ten years. We have, at any rate, introduced a thread of unity in our explanation of the laws of rent, wages, profit, and interest. The application of the ideas of marginal utility and marginal productivity have done much towards enabling us to reduce several of these laws to a common principle.